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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 2 NOVEMBER, 1939 No. 1

SECTION OF GEOLOGY AND MINERALOGY

OCTOBER 2, 1939

MEREDITH E. JOHNSON, State Geologist, Trenton, N. J.: *Unsolved Problems of New Jersey's Geology.* (This lecture was illustrated by lantern slides and motion pictures.)

In 1836, Henry D. Rogers published his first report on the geology of New Jersey and this was followed in 1840 by his "final report." At that time this latter report doubtless *was* the last word on the subject; yet the need for more detailed knowledge of the geology of the State soon became evident, and I do not think anyone would seriously contend that the many reports on geology which followed were not needed, or that they did not help in the development and utilization of New Jersey's natural resources. It is unfortunate that in the last twenty years the idea that New Jersey's geologic problems have all been solved has gained so much ground that even our legislators are laboring under that impression. Perhaps the publication of the geologic map of the State in 1912 did as much as anything to foster this idea. It is a good map, published on a scale of four miles to an inch, and for many purposes it has been entirely adequate. But it does not tell us the chemical variations in the limestones whose areal distribution it shows;

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it does not show where shale suitable for the manufacture of brick occurs; or where material suitable for the manufacture of mineral wool may be found. In short, it does not provide the answer for a great many questions to which increasing industrial development has led; nor does it answer the more academic problems which are of immediate interest to all geologists and the answers to which—if we may judge from past experience—will ultimately prove of practical as well as scientific value. In discussing some of the more interesting of New Jersey's unsolved geologic problems, it is my hope to attract attention to the need for additional geologic work, and the further necessity of making that need known if we are to progress in clearing up our geologic mysteries.

Problem 1. What is the age and what are the relationships of the igneous and metamorphic rocks which cross Delaware River at Trenton?

This is an old and hard nut to crack. The Pennsylvania Survey has been laboring with it mightily for many years, but still the answers to the several questions at issue are not sufficiently clear to be acceptable to all.

The rocks chiefly involved here are of three general types; namely, a light-colored granite-gneiss which I am glad to say is apparently accepted by everyone as Pre-Cambrian in age, and which is of the same general type as similar rock in Maryland which has been given the name, "Baltimore gneiss." North of a pronounced fault which runs through Trenton, Langhorne Station, Trevose and points farther to the southwest, the granite-gneiss is overlain stratigraphically by a hard, typically pale-green quartzite and that by light-colored dolomitic limestone. Though not stratigraphically continuous with the similar sedimentary beds near New Hope and Norristown, the gaps are not wide, and there is general agreement again that all these beds are to be correlated with the Chickies quartzite of Cambrian age, and the Shenandoah or Kittatinny limestone of Cambro-Ordovician age. But there agreement ends, for on the south side of the aforesaid fault we find a metamorphosed sediment—the Wissahickon formation—that

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by some is considered of Pre-Cambrian age, and by others, Ordovician.

You are all familiar with the arguments involved. The proponents of the Pre-Cambrian hypothesis explain the absence of the thick Wissahickon formation north of the fault by saying that the present contiguous position of unlike series of rocks is due to a great thrust—the Martic overthrust—which has brought together rocks that formerly were many miles distant from one another, and that either (1) the sea in which the Wissahickon sediments were deposited did not extend as far to the northwest as the line of the fault; or (2) that erosion subsequent to their deposition removed them prior to the deposition of the Paleozoic sediments.

The opponents of the Pre-Cambrian theory point to the fact that the line of the fault is relatively straight; whereas, if the angle of the thrust is anything less than 45 degrees, there should be decided offsets where Neshaminy Creek, Pennypack Creek, Wissahickon Creek and Schuylkill River have cut gorges through the resistant quartzite and granite-gneiss. They point also to the similar rock succession southeast of the Martic overthrust at Avondale where quartzite and marble directly overlie the Baltimore gneiss, and the Wissahickon formation admittedly overlies both.

In rebuttal, proponents of the Pre-Cambrian hypothesis can point out that whereas gabbro of identical appearance cuts both the Baltimore gneiss and Wissahickon formations in widespread intrusions, nowhere can it be found cutting the Chickies and Kittatinny formations, or still younger rocks.

Additional arguments for both sides could be cited, but in view of the many other problems which I wish to mention, I will have to proceed.

Problem 2. Girard Wheeler[1] has recently published a paper in the *Journal of Geology* on "Triassic Fault-line Deflections and Associated Warping" which throws new light on another interesting problem: namely, the peculiar crescentic

[1] Wheeler, Girard. Triassic Fault-line Deflections and Associated Warping. *Jour. Geol.* 17 (4): 337–370. May–June, 1939.

shape of the several flows and intrusions of trap adjacent to the western border of the Triassic basin in New Jersey. These flows and dikes had previously been studied by Darton, Kummel and Lewis, but according to Wheeler, previous to his study no one had offered a satisfactory explanation of their shape and structure.

In studying the problem, Wheeler found an apparent relationship between irregularities in the fault boundary and the structure of the adjacent Triassic rocks. Where salients of the older rocks project into the younger, the downfaulted Triassic rocks are warped into anticlines. Opposite re-entrants, synclinal folds occur. The throw of the Triassic rocks opposite salients according to this theory is a minimum, whereas at re-entrants, the maximum throw occurs.

In his paper, Wheeler gives many good sketches to illustrate his theory but not all of these seem to confirm it. For example, on page 353 it will be noted that the prongs of the Sand Brook crescent do not face the re-entrant in accordance with theory.

The small basaltic sheet at Flemington is an area of trap which Wheeler did not discuss but which is of interest because of the surprising thickness of trap recorded in the well which is shown in the southern part of the area. This well starts at elevation 263 and not more than 45 feet above the lower contact of the trap with the surrounding shale; yet this same contact was not reached in the well until the drill had reached a depth of 178 feet. Since nowhere along its outcrop does the trap reach such low elevation, the conclusion seems inescapable that the under contact is dipping inward from all sides. This cone-shaped structure is interpreted as the remnant of a small basaltic plug which probably is connected at depth with the diabase dikes which outcrop nearby.

In the anticlinal trap sheet at New Vernon, again there is poor agreement with Wheeler's theory, yet, as he has pointed out, it may well be that beneath the present erosion surface, the fault line has greater convexity.

Wheeler did not discuss the structure of the diabase sill constituting Cushetunk Mountain, but it is so similar in shape and manner of occurrence to the sills and sheets he did describe that it certainly is part and parcel of the same general problem. In this case the trap cuts across the enclosing beds of shale and even close to the great boundary fault the dips seem to bear little relation to it. Is it just coincidence that this great sill was intruded in the form of a crescent with a detached small central plug, or does the boundary fault here too bulge outward at depth in a manner not shown at the surface; and did that bulge antedate the intrusion of the diabase and influence its disposition?

Problem 3. Frequently I am asked to classify specimens of hard, gray, argillaceous rock which at first glance may be either metamorphosed shale, or argillite. The occasional difficulty in giving a satisfactory reply always reminds me of another problem: namely,—What differences in character of sediment or in environmental conditions produced argillite from the muds deposited in the central part of the Triassic basin; whereas the muds deposited to the northeast and southwest were converted into relatively soft shale? Was there actually a difference in the materials deposited? Was there a difference in the chemical content of the ground water which circulated through the different parts of the Triassic sediments; or is some other cause responsible for the great difference in the present hardness of these similar argillaceous beds?

Problem 4. Where is the contact in New Jersey between Cambrian and Ordovician strata?

We know that the Conococheaque limestone of Pennsylvania, which seems to be the correlative of the middle portion of our Kittatinny formation, contains Cambrian fossils—and indeed a few such fossils have been found in beds in the lower and middle parts of the Kittatinny in New Jersey—but fossils in these beds are so scarce that to date we have had little success in attempting to pick out the uppermost bed of Cambrian age.

The same story is true of the overlying dolomitic beds of Beekmantown age. Occasional fossils have been found in the lower part of these beds, but they are few and far between, so the most that we can say at present is that apparently the Beekmantown formation is at least 1700 feet thick in the vicinity of Phillipsburg. In that area, John Hills, while at Lafayette and in subsequent post-graduate work, made stratigraphic studies which included a microscopic examination of limestone residues. To him should go the credit, I believe, for first noting the presence in such residues of very simple forms of foraminifera, and this discovery should serve to help unravel this problem. Dr. Benjamin Howell of Princeton has for several years been working on this same problem and it is hoped that eventually, by the use of all possible aids, we may be able to establish enough criteria to enable us to recognize the correct stratigraphic horizon even of isolated outcrops.

Problem 5. Where should we draw the line between Silurian and Devonian strata in New Jersey?

These strata are well exposed in only a few localities in the northwestern part of the State where they were first studied in detail by Stuart Weller about 1900. In his report on Paleozoic Paleontology he stated clearly that the faunal zones he had found were similar to those in southeastern New York and he followed the New York nomenclature in naming the stratigraphic units. In placing the line between the Silurian and Devonian strata at the contact between the Manlius and Coeymans formations, as had been done in New York, he did not overlook the peculiar Rondout fauna, composed almost exclusively of members of the genus Leperditia, or the abundance of ostracodes in the Manlius and Decker beds. Instead, he did so because of his expressed opinion that "the greatest faunal change is to be found in passing from the Manlius to the Coeymans limestone, and the Coeymans fauna represents the first distinctive immigration of an important, typically marine fauna into the northern portion of the Cumberland basin since its occupation by the Decker Ferry faunas. The importance of

this immigration seems to be sufficient to be recognized as the beginning of the period which we call Devonian time, the Coeymans fauna being the earliest of the Helderbergian faunas in America, all of which have distinctly Devonian characteristics."

I believe that the New York Geological Survey still concurs in Weller's opinion. Recently, however, the Pennsylvania Survey has published "A Paleozoic Section at Delaware Water Gap" by Bradford Willard in which the Decker, Rondout and Manlius are all placed in the Devonian although the Silurian-Devonian contact was admittedly "still a bone of contention in Pennsylvania."[2]

In this connection, Frank C. Whitmore, Jr., who recently finished a very thorough re-study of the fossiliferous beds at the Nearpass quarry in Sussex County, has written me as follows: "In view of this confusion (with respect to the Silurian-Devonian contact), I decided to go as far as I could on what evidence I could find myself. Of course, the age of the Manlius, Rondout and Decker is dependent upon the age of the Keyser, with which it is undoubtedly roughly equivalent; and it is my opinion that the Keyser faunas show a Silurian tendency. I believe that a Silurian age for the Keyser is further indicated by the widespread unconformity in Pennsylvania, Maryland, etc., between the Keyser and the overlying Coeymans. Needless to say, I do not regard the problem as solved, as I know that excellent faunal arguments can be formulated in support of either the Silurian or Devonian age of the Keyser. I drew my own conclusions from what seemed to me to be the strongest faunal evidence; especially that of the ostracoda."

To the writer, the evidence of a stratigraphic break at the top of the Keyser, together with the accumulated faunal evidence, point to the ultimate recognition of the Coeymans as the basal Devonian formation; but the problem certainly cannot yet be called "settled."

[2] Since the oral presentation of this paper, Dr. Willard has informed the writer that the Pennsylvania Survey, in its forthcoming bulletin on the Devonian strata of Pennsylvania, has placed these beds in the Silurian.

Problem 6. What is the age of the Manasquan, Vincentown and Hornerstown formations?

William B. Clark was the first to classify the coastal plain sediments on the basis of their contained fossils, and he included everything from the Manasquan down in the Cretaceous. In 1907, after several years of intensive field work, Stuart Weller published his excellent and thorough treatise on the "Cretaceous Paleontology of New Jersey." In this he found occasion to differ somewhat with the stratigraphic divisions made by Clark, but he concurred with him in placing the Manasquan and all older unconsolidated sediments in the Cretaceous, even though the marked unconformity at the base of the Hornerstown marl was then known. It was not until 1928 that this classification was seriously questioned. In that year, however, C. W. Cooke and Lloyd W. Stephenson[3] made a new analysis of the fauna from the Hornerstown, Vincentown, Manasquan and Shark River formations which, together with the aforementioned unconformity at the base of the Hornerstown, seemed to strongly indicate the Eocene age of all four formations.

In 1930, Alexander Wetmore[4] reviewed the stratigraphic occurrence of fossil birds from supposed Cretaceous beds in New Jersey and concluded that all the species known were of Eocene aspect, and, hence, that the Hornerstown marl, in which they had all been found, was of Eocene age. This confirmation of Cooke and Stephenson's conclusions was very pleasing to all those who wished to see this problem definitely settled; but in 1933 the applecart was again upset when Ferdinand Canu and Ray S. Bassler,[5] after a study of the bryozoan fauna of the Vincentown formation, stated their opinion that these bryozoa were more like the Upper Cretaceous than the Eocene forms

[3] Cooke, C. Wythe, and Lloyd W. Stephenson. The Eocene Age of the Supposed Late Upper Cretaceous Greensand Marls of New Jersey. *Jour. Geol.* 36 (2). Feb.–March, 1928.

[4] Wetmore, Alexander. The Age of the Supposed Cretaceous Birds from New Jersey. *The Auk* 47 (2): 182–186. 1930.

[5] Canu, Ferdinand, and Ray S. Bassler. The Bryozoan Fauna of the Vincentown Limesand. U. S. Nat. Mus., Bull. 165. 1933.

of Europe and that they bore little resemblance to the usual Tertiary faunas of America.

Since then, A. K. Miller and M. L. Thompson[6] have lined up on the Tertiary side of the argument as a result of their study of the nautiloid genus *Aturoidea* in America; and Philip H. Jennings[7] has added confirmatory evidence as a result of his study of microfauna from the Monmouth group (Red Bank, Navesink and Mount Laurel-Wenonah formations) and the Hornerstown marl. Also, Stephenson[8] has cited additional faunal evidence in support of his case as the result of a re-study of boring mollusks found in the Vincentown and subjacent formations.

To an impartial geologist, whose only interest is in seeing the problem settled one way or another, the preponderance of evidence seems to favor the supposition that the Hornerstown, Vincentown and Manasquan formations are Eocene in age; but he has recently been reminded that certain dinosaur remains were found in these beds and that the dinosaurs are supposed to have become extinct in late Cretaceous time. So the problem is evidently still far from settled.

This paper could be greatly extended by discussing all of the following problems, but as my intention was only to arouse an interest in their settlement, I will be content merely to list them:

Problem 7. What is the age of the Manhattan schist formerly exposed at low tide in Jersey City, and of the serpentine which apparently intrudes it?

Problem 8. To what extent has thrust faulting displaced the strata of the Highlands region? For example, we know that at West Portal the Pre-Cambrian gneiss has been thrust northwestward for at least one mile. Is this a maximum, or

[6] Miller, A. K., and M. L. Thompson. The Nautiloid Genus *Aturoidea* in America. *Jour. Pal.* 9 (7): 563–571. 1935.

[7] Jennings, Philip H. A Microfauna from the Monmouth and Basal Rancocas Groups of New Jersey. *Bull. Amer. Pal.* 23 (78): 3–76. 1936.

[8] Stephenson, Lloyd W. The Stratigraphic Significance of *Kummelia*, a New Eocene Bivalve Genus from New Jersey. *Jour. Wash. Acad. Sci.* 27 (2): 58–64. 1937.

is the throw still greater at other points where evidence bearing on the problem is now lacking?

Problem 9. What forces produced the complicated structural pattern north of Flemington, where the Triassic and Paleozoic sediments have been twisted around so that they strike at right angles to the normal NE-SW structural trend?

Problem 10. How far to the southeast do the Paleozoic strata extend beneath their present cover of Triassic sediments?

Problem 11. What is the genesis of the zinc deposits at Franklin and Ogdensburg?

Problem 12. What was the source of the copper minerals at the old Pahaquarry Mine near Dimmicks Ferry, Delaware River, which is many miles distant from the nearest known intrusion of igneous rock? In what age did mineralization occur?

Problem 13. What is the origin of the depressions in the relatively flat surface of the Pensauken formation between Trenton and South Amboy, and occasionally seen in level ground, underlain by other formations?

Problem 14. What is the age and origin of the Cape May formation, the Pensauken, the Bridgeton, and the Beacon Hill?

Problem 15. How many glacial ice-sheets over-ran northern New Jersey? Did the ice-front slowly recede at the end of each glacial epoch with occasional halts and re-advances, as believed by Salisbury and Antevs; or was the maximum advance followed in each case by stagnation of the ice-sheet with gradual disappearance due to melting in place, as believed by Flint?

Problem 16. What was the maximum westward extension of the sediments of the Coastal Plain?

Problem 17. Do the headwaters of any of our major streams antedate the Cretaceous sediments, and if so, which ones?

Problem 18. How many peneplains have been developed in New Jersey and what is the evidence?

These are some of the more important unsolved geologic problems that have occurred to me, but I make no pretensions that the list is complete. Probably any one familiar with the geology of New Jersey could name half a dozen more without much thought. For the sake of brevity, however, I shall bring this paper to an end with a brief statement of what is being done to solve one final and related group of problems: namely,—

Problem 19. What is the structure of the sediments constituting the Coastal Plain? What is the depth to the rock floor beneath the sediments? Of what kinds of rock is the rock floor composed?

The best way to get data about materials below the surface of the ground is to drill test holes. By preserving samples of the materials encountered and keeping a careful record of the depth at which each change occurs, an accurate picture of the underground strata can be obtained. For that reason the New Jersey Geological Survey has from its inception attempted to obtain and preserve samples from deep wells in areas where information pertaining to the sub-surface geology is lacking. But deep wells are expensive, and unless there is some practical purpose to be served, such as the obtaining of a ground-water supply, no drilling is done and the geologist must seek his information in other ways. That is why geophysics has come to play an increasingly important role as an aid to the geologist; for information can now be obtained by geophysical methods which would be exorbitantly expensive if it had to be obtained by drilling.

Because the evidence accumulated from wells in recent years had thrown some doubt on the old conception that the sediments of the Coastal Plain had undeformed, monoclinal dips to the southeast, the opportunity to aid Dr. Maurice Ewing and Dr. George P. Woollard in the fall of 1937 in making a survey by seismic methods of a line across the Coastal Plain was eagerly accepted. It was thought desirable in running this first seismic profile to have an indisputable check upon the results. Consequently, it was run through Jacksons Mills where

a deep well had been drilled far into the basement rock. Also, it was run at right angles to the strike of the sediments in order to determine their maximum dip. As fully described in papers which have appeared in the *Transactions of the American Geophysical Union*[9] and the *Bulletin of the Geological Society of America*,[10] the results of the survey were most gratifying. Not only was the dip of certain recognizable horizons determined accurately, but also the depth to the rock floor was determined for the entire line of the profile; a hitherto unsuspected fault block was mapped near Plainsboro; and the nature of the underlying crystalline rock was determined on the basis of rock densities and structural trends.

A second seismic profile across the Coastal Plain was run in the summer of 1938 from Bridgeport to Avalon in Cape May County; and this line also was run at right angles with the regional structure. Results again were highly satisfactory, although inability to reach basement with the shots at Seaville and Avalon was somewhat disappointing. This work showed an average dip of the rock floor of about 85 feet per mile as compared with 65 feet per mile along the more northerly profile, as well as the same steepening of the dip near the present shoreline. The depth to basement at Woodbine, the most easterly of the stations to reach bed rock, was determined to be 4,592 feet. For other results, I am going to refer those who read this to Part IV of "Geophysical Investigations in the Emerged and Submerged Atlantic Coastal Plain," which is due to appear soon in the Bulletin of the Geological Society of America.

Another geophysical aid was invoked through the U. S. Coast and Geodetic Survey, who very generously extended their net of gravity stations in northern New Jersey in 1937, and in 1938 occupied stations along the lines of both the afore-

[9] Woollard, George P., Maurice Ewing and Meredith E. Johnson. Geophysical Investigations of the Geologic Structure of the Coastal Plain. *Trans. Amer. Geophys. Union.* Nineteenth Annual Meeting. 1938.

[10] Ewing, Maurice, George P. Woollard and A. C. Vine. Geophysical Investigations in the Emerged and Submerged Atlantic Coastal Plain, Part III. Barnegat Bay, New Jersey, Section. *Geol. Soc. Amer. Bull.* 50: 257–296. 1939.

mentioned seismic profiles as well as certain detached stations which had been requested. The results of this gravity work have been plotted through points having the same gravity anomaly, and I think that I am safe from contradiction in saying that some of the anomalies shown are very suggestive. For example, the lines denoting equal anomalies which run from Bordentown to the northeastern corner of the State come very close to following the eastern boundary of the Triassic rocks. Likewise, the high negative anomalies observed in eastern Pennsylvania and Vermont appear to be directly related to the steeply folded Paleozoic rocks of the Appalachian geosyncline. The change to positive anomalies in the central part of New Jersey would seem to indicate the presence of high-density rocks such as the gabbro which has so permeated the Wissahickon formation. Other inferences are possible, but the relation between structure and gravity is not always clear and I will, therefore, let my readers draw their own conclusions as to the significance of the high negative anomalies in Cape May County and the possible relationship between the closely spaced isostatic anomalies passing through Philadelphia and Trenton,—representing a rapid transition from positive to negative anomalies—and the earthquake which severely shook those cities a year or so ago.

Finally, during this past summer, one other geophysical aid was invoked. With an Askania magnetometer and such aid as the writer could find the time to give, George Woollard has mapped the position of the buried Palisades diabase between Sayreville and Deans, and has extended the network of his magnetometer readings both to a considerable area in the neighborhood of Plainsboro and Princeton Junction, and also along the northerly profile from Plainsboro to Mantoloking and beyond. These readings of the vertical intensity of the earth's magnetic field should help materially in the interpretation of the gravity and seismic data already obtained, and I am going to suggest that if this Section of the Academy can get Woollard to explain the significance of his field data at some future meeting, it will be rewarded by a most interesting paper.

SECTION OF BIOLOGY

OCTOBER 9, 1939

JEROME ALEXANDER, Consulting Chemist, New York, N. Y.: *Physico-chemical Determinism in Biology.* (This lecture was illustrated by the use of opaque projection apparatus and slides.)

Berzelius' view that catalysts owe their potency to what we now term their outwardly directed residual electronic fields, is a basis for understanding the extreme specificity of catalysts in general and of enzymic biocatalysts in particular. The potency of molecular electronic fields is great, but highly localized, varying approximately as the ninth power of the distance (Langmuir). Before reactants can be sufficiently affected by the electronic fields of a catalyst, they must enter its electronic jurisdiction, near enough to be distorted by these fields of force, somewhat as the opposing hooks of a "zipper" are twisted into position to unite by the "key."

Commercially, catalysts are used to synthesize a wide variety of organic and inorganic compounds on an enormous scale. The kind of catalyst used determines the nature and percentage of substances synthesized. Introduction of new atoms or molecules into the catalyst surface may stimulate, inhibit, or alter the nature and rate of the catalyzed reactions. Generally, any substance which changes the behavior of a catalyst may be called a "modifier," and exceedingly minute amounts of materials serve this purpose.

Up to a certain point, increase in temperature will facilitate reaction by bringing to the catalyst more reacting particles per unit of time but beyond this point the number of particles exceeding the critical speed limit will increase so greatly that catalyst efficiency will fall off; hence the optimum temperature of action so commonly observed for biocatalysts.

The genes and enzymes which largely direct the course of chemical change in living beings are catalyst particles of col-

loidal dimensions. Some molecules may be of such size that they are born colloids; some achieve colloidality by molecular growth or by aggregation; and some have colloidality thrust upon them by adsorption or chemical fixation on a colloid particle or surface. If the fixed particle can form a specifically active catalytic area after fixation, it is termed a *prosthetic* or activating group. The word "prosthetic" is derived from a Greek root meaning to add to, or to insert. Thus prosthetic dentistry deals with the insertion of teeth to replace missing ones. A prosthetic group puts "teeth" into the catalyst.

Decrease in the size of a particle results in a great increase in its kinetic activity. Toward the lower range of the colloidal zone where the specific surface (free surface per unit weight) is still considerable, particulate kinetic activity becomes ultramicroscopically visible; whereas to use an "Irish bull," if we could see a hydrogen molecule we could not see it, because it would move too fast—with about the speed of a rifle bullet. That degree of smallness of particle where kinetic activity is insufficient to nullify or to obscure the consequences of specific surface, has been termed the *zone of maximum colloidality.*

Life involves two essential activities: self duplication or reproduction and the catalytic direction of chemical change. These two criteria would be met by an autocatalytic catalyst, that is, a catalyst which catalyzes its own formation. In addition, most, if not all living units that we know, are capable of undergoing changes (*e.g.,* differentiation, mutation) which alter the nature of their catalyzed or metabolic products, and which may be passed on to their descendants. Genes, bacteriophages and viruses are examples of the smallest known living units. They are sub-microscopic colloidal particles of widely varying dimensions, evidently extremely large molecules or comparatively small molecular groups. Given a suitable milieu, such units reproduce and direct chemical change. According to this view, an enzyme capable of self-duplication would have to be considered alive, and the simplest conceivable living unit would be a *moleculobiont,* (Alexander & Bridges, 1928) a molecular

catalyst, which, given a suitable milieu, is capable of self-duplication. When we have not succeeded in finding a laboratory milieu in which to propagate a biont which grows parasitically upon or within other bionts, it is common to refer to the invader as an *obligate parasite.* The term *obligate* is merely a confession of temporary ignorance, and not the expression of an experimentally warranted conclusion; for the production of suitable synthetic media should be regarded as unsolved rather than insoluble problems.

To understand the ultimate basis of life within a cell, we must concentrate our attention upon the simplest self-duplicating units—chromosomes and their constituent genes, mitochondria and possible sub-cellular symbionts. The cytoplasm, apart from these, is a pool of localized milieu, receiving and contributing special molecules, an arena for the activities of the enzymes and catalytic surfaces. The ultimate living units in a cell are the self-duplicating ones. The view that protoplasm is the "living jelly" is a relict of superannuated textbooks, and should be definitely abandoned.

Both enzymes and viruses have been crystallized and appear to be proteins. In their natural state they are dispersed colloids and are probably prevented from exercising their natural tendency to establish a visible crystal lattice by the presence of protective or deflocculating colloids and by the other conditions (*e.g.,* ionic concentrations) of their natural milieu. Special procedures are required to purify and crystallize them. The crystallization of substances heretofore believed to be non-crystallizable has astonished many who fail to understand that orderly arrangement in a space lattice is a potential minimum which molecules and molecular groupings tend to assume, although circumstances prevent, and some intermediate stage persists. We have many crystalline colloids—Scherrer showed that the individual particles of colloidal gold are tiny crystals.

Proteins are subject to aggregative changes apart from actual crystallization. Thus Eriksson-Quensel and Svedberg found that the molecules in a solution of the hemocyanin from

the snail *Helix pomatia,* show a molecular weight of 6,740,000 when slightly acid (pH 6.8); but when made slightly alkaline (pH 8.0) the protein splits into three components whose molecular weights are 6,740,000, 3,370,000 and 842,000 respectively. On restoring the pH to 6.8, all the fragments of dissociation completely unite to form the original component. All vertebrates except the Cyclostomata have a protein blood corpuscle pigment of the same molecular weight, 68,000 (hemoglobin) with four iron atoms per molecule. With Cyclostomata the corpuscle pigment has a molecular weight only one-fourth that of hemoglobin. "The molecular weights of the hemocyanin molecules found in the blood of certain species are always simple multiples of the lowest well-defined component. Thus, for the Malacostraca the relationship is 1:2 and for the Gastropoda 2:8:16:24. Moreover, the weights of all the well-defined hemocyanin molecules seem to be simple multiples of the lowest among them. In most cases the hemocyanin components of certain species are interconnected by reversible, pH-influenced dissociation-association reactions. The shift of pH necessary to bring about reaction is not more than a few tenths of a unit. Consequently, the forces holding dissociable parts of the molecules together must be very feeble."

"Not only the molecular weights of the hemocyanins but also the mass of most protein molecules—even those belonging to chemically different substances—show a similar relationship. Probably the protein molecule is built up by successive aggregation of definite units, but only a few aggregates are stable. The higher the molecular weight the fewer are the possibilities of stable aggregation. The steps between the existing molecules, therefore, become larger and larger as the weight increases." (Svedberg, *Ind. Eng. Chem., Anal. Ed.,* 1938, **10**, 113–129.)

Since few, if any, biological substances above the molecular or macromolecular orders occur in a state of purity, we must consider some aspects of the behavior of mixtures as contrasted with the behavior of pure substances. Mere traces, too often regarded as "negligible impurities" or as the "undetermined"

amount necessary to bring an analysis to 100 per cent, may exert surprising influence, apart from serving as prosthetic groups in enzymes. Such secondary constituents, adsorbed at specific particulate surfaces, often act as protective or deflocculating colloids, opposing the formation of larger aggregates. Frequently, however, they may cause the coated particles to *cohere,* and may then be termed *cohesive colloids.*

With very small surfaces chemical specificity becomes increasingly dominant. Thus adsorption of antibody by antigen is highly specific, though there may be some overlapping or cross-precipitation. Where bacteria become coated with an adsorbed layer of their specific antibody, and then approach closely, the antibody serves as a cohesive colloid and binds the bacteria into a clump of such size that Brownian motion ceases and the clumps gather together and flock out. Topley, Wilson and Duncan showed that on agglutinating a heterogeneous mixture of bacteria by a heterogeneous mixture of specific antisera, each cluster of bacteria is homogeneous. Lindegren and Bridges invoke this principle to explain synapsis, which "would proceed zipper-like, throughout the entire length of the chromosome."

Without the intervention of some mechanism capable of directing orderly differentiation, the rapidly segmenting zygote would become a mass of cells of one kind. The formation of a globular mass of cells establishes a *diffusion gradient* between cells on the interior. The external cells are more advantageously situated for chemical interchange with the milieu and tend to duplicate more rapidly than the cells blanketed within the mass. This means that the cell mass must develop more external surface, which, unless the cells disaggregate, can happen only in one of the two ways: (1) the surface cells may break loose from the main mass and form a *diverticulum,* as is common with plants, *e.g.,* in roots and hyphae; (2) if the cells cohere, the struggle for additional free surface leads to an *invagination,* and we have the gastrula form characteristic of animals. The development of a gastrula automatically en-

forces the development of three ionic domains: (1) an ectoderm, in contact with the external "ocean" of milieu; (2) an endoderm, in contact with the gastrulitic "gulf or bay"; (3) a mesoderm, intermediate between the other two domains. With the establishment of these ionic differences we can understand how specific molecules can be selectively aggregated or adsorbed by cellular catalysts within the cells of the several domains. The catalysts (genes and/or enzymes) *thus undergo modification* and the chemism of the cells undergoes a corresponding diversion, which leads up to and becomes evident as differentiation. The modifying particles apparently accumulate in the so-called gray crescent at the mouth of the gastrula, from which, as Jennings puts it, an organizing influence sets out. The relation to the *organizator* of Spemann and the evoker of Needham is at once evident. The specific molecules needed for the modification of the cellular catalysts at this stage, though large in number, are so small in mass that they would readily have been carried in the cytoplasm of the original zygote. Though there are billions of diverse but highly specific molecules free within the zygote cytoplasm or adherent to the chromonema, it is not necessary to assume that these must be sufficient to modify all the catalysts in larger adult organisms. The specific modifying particles of the zygote may serve as moulds or templets for the formation of duplicates of themselves. A self-duplication of this kind evidently occurs with bacteriophage, viruses, and mosaics, though some of these may have a supermolecular structure. To illustrate this mechanism, a piece of metal foil is pressed to the surface of a coin. The foil on the face *next* to the coin acquires a surface contour precisely opposite to that of the coin; but the outer surface of the foil shows a surface contour *precisely the same* as the coin. Thus a catalytic mould could duplicate its own specific surface as well as produce a specifically reverse surface, depending upon which surface remains facing the milieu, following adsorptive or other fixation of the liberated molecular or macromolecular plaque. The work of Langmuir, Adam, Wrinch and others shows the thinness and top-and-bottom specificity of surface films.

If the normal course of life is in part directed by normal catalyst modifiers, the introduction of abnormal modifiers may so divert the course of catalysis as to give rise to functions or structures which could be termed pathological. For example in methylcholanthrene we have a definite chemical substance which incites cells to duplicate themselves and to invade healthy tissue, a condition which clinicians identify as cancer. Since cancer cells duplicate themselves specifically, the catalysts and/or modifiers responsible for abnormal cell behavior must also be duplicated. Whatever the remote cause, the proximate cause of cancer appears to be a heritable change in biocatalysts either by way of mutation or of modification.

SECTION OF PSYCHOLOGY

OCTOBER 16, 1939

PROFESSOR HORACE B. ENGLISH, Ohio State University, Columbus, Ohio: *Experiments in Substance Memory.* (This lecture was illustrated by lantern slides.)

The learning of everyday life consists in large part of acquiring the "substance" or meaning of something studied. This type of learning is little investigated and even little illuminated by traditional experimental procedures. This is partly because traditional theory minimizes meaningfulness, partly because it is difficult to experiment with meaning. For as soon as one contrasts something having less meaning—rote memory material, say—with something having rich meaning, one introduces other differences of unknown effect on the learning process.

We have sought to avoid this difficulty by contrasting as principal experimental variables, not materials with different degrees of meaningfulness, but two kinds of learning outcomes which reveal—and require—different degrees of acquisition of meaning.

Specifically, we use a battery of recognition questions. Half of these can be answered in virtue of a purely verbatim, rather rote-like retention. The other half can only be answered if one has grasped the meaning or substance of the prose passage used. Both sets of questions are based on the same passage. One uses the same subjects under the same learning conditions for both sets of questions.

Over two million individual responses have been recorded under a variety of experimental conditions. Special attention has been focused on the phenomenon of reminiscence over periods ranging from ten minutes to ninety days. This paper deals with a methodological problem involved in one typical experiment.

Reminiscence seems to play a more significant rôle with connected prose than with nonsense syllables. Average scores based on the total test usually show a rise after twenty-four hours, followed by a slow decline. When the test is divided into its two components, the test requiring the greater amount of meaning-understanding shows no forgetting; instead there is slight but consistent gain. Apparently rote memory is subject to much forgetting, grasp of significance to none at all within the ninety day period. All the forgetting in average scores is localized in the half of the test which rests primarily on verbatim retention.

This, however, is a matter of averages. But forgetting and reminiscence are matters of the patterns of individual responses to specific memory tasks—in this case of the permutations of right and wrong responses by a particular subject to a particular test item at different times. For this the statistics of central tendency are misleading; we need what we may call statistics of categories. Tabulation of the response-patterns in a four way table, Forgetting-Reminiscence and "Much Meaning"-"Less Meaning" still showed forgetting correlated with the more rote-like memory task.

Since, however, reminiscence and forgetting are relative to the earlier response, we should count, not the absolute, but the relative amount of reminiscence in the two halves of the test. And by the criterion of per cent of possible chances, almost exactly as many items are forgotten in each sub-test—a conclusion almost directly opposite to that previously reached.

Further analysis to resolve the contradiction took the form of a simplified analysis of the variance to eliminate the intrusive effect of differential initial difficulty. By this means, it is shown that the correlation between reminiscence and meaningfulness holds after all. This conclusion was confirmed in a later study in which initial difficulty was held constant not merely statistically but experimentally.

The need for a methodological critique is clear if we review the series of conclusions and their bases:

1. Logical memory shows first a rise and then loss with lapse of time. (Conclusion based on total test averages.)
2. Retention of just the substance or idea of a passage shows no loss with time but rather reminiscence. All the forgetting found in the total test is localized in the half-test which stresses verbatim retention. (Based on sub-test averages.)
3. There is the same *relative* amount of forgetting in the two sub-tests. (Based on the per cent of possible manifestations.)
4. There is an equal amount of forgetting in the two sub-tests, but more reminiscence in one than the other. (Based on an analysis of individual patterns of response, equating for initial difficulty.)

But the more careful the analysis, the more certain it seems to be that the two sub-tests are really measuring distinct processes. The consistency of the association or correlation of discrepant retention values with relative meaningfulness is most parsimoniously explained in terms of at least two distinct, though probably inseparably linked, organic processes; the one process mediating a rote-like memory process and conditioning, the other mediating the development of meaningful understanding.

Whether, however, this hypothesis is ultimately confirmed or not, it is contended that on the merely descriptive level there are disclosed too many differences between richly meaningful memory and the meaning-impoverished memory of traditional experiment for the latter to be considered the pattern of the former. On both theoretical and practical grounds the time has come when the kind of learning most used in everyday life (the learning of the "substance" of what is presented) must be systematically investigated.

SECTION OF ANTHROPOLOGY

OCTOBER 23, 1939

DOCTOR MARGARET MEAD, American Museum of Natural History: *Researches in Bali, 1936–1939.* (This lecture was illustrated by motion pictures.)

I. *On the Concept of Plot in Culture*

Anthropology is such a young science and its materials are so rich and varied that the study of almost every culture suggests to the investigator new approaches to the material, and approaches which are often not immediately applicable to other cultures. Although eventually every approach, every method of analysis which has proved fruitful in the study of a particular culture must be systematized so that it has cross cultural significance, and the absence of a special type of relationship between cultural phenomena must be integrated with the presence of this relationship elsewhere, the fact that this systematization is not yet possible need not discourage us from using as completely as possible the special leads which given cultures provide. Bali is an extremely rich culture, rich in symbolic forms which may be studied in relation to the type of personality which they express on the one hand, and help to create on the other.

Certain forms of trance in Bali illustrate particularly well one of these special approaches to the analysis of cultural forms, the idea of "plot," originally sketched out in almost allegorical terms by Freud,[1] and later reduced to terms more consistent with cultural diversity by Roheim.[2] This approach suggests a close and patterned relationship between a type of experience in early childhood, which is standardized for a given culture, and the ritual or other symbolic forms found in the same cul-

[1] Freud, S. *Totem and Taboo.* New York: Moffat, Yard and Co., 1918.

[2] Roheim, G. *The Riddle of the Sphinx.* London: The Hogarth Press, 1934. See also Mead, M. Review of the above in *Character and Personality,* September, 1935.

ture. This conception proves exceptionally fruitful in some cases; we do not yet know whether it is applicable to all cultures and caution must be observed even in its application to the analysis of a given culture.

Bali shares with many other regions of the world an emphasis upon trance states, as a way in which the will of extranatural beings, the dead and the Gods, communicate through possessed persons with human beings. In many of its forms Balinese trance does not differ in any conspicuous way from trance in other parts of Oceania and even in other parts of the world. It is possible to distinguish various types of Balinese trance, from the simple spontaneous trance in which a private, unsanctified and uninstitutionalized person falls down in a faint and makes associated comments which may or may not be accepted as having supernatural import, through institutionalized seers and oracles, who play an important rôle in providing the slight impetus towards change, towards the adoption of new forms of rituals, building new temples, consecrating new religious officials, etc., which is permitted within the rigid and static structure of Balinese society. There is another important variety of trance, in which the performers are called *sangijangs,* and go into states preceded and followed by short deep trances, during which, in a condition which seems to parallel somnambulistic states induced by hypnosis, they perform a variety of stereotyped dances and dramatic representations. Although the details of the *sangijang* forms reflect important aspects of Balinese culture, they apparently do not necessarily depend upon any particular traumatic childhood experience, and would presumably be possible to any child who had been exposed to the general impact of Balinese culture. I mean exposure in the sense that Chinese and Europeans living in Malay regions sometimes manifest the peculiar form of madness known as *amok* which is characteristic of that region of the world.[3] The proportion of individuals able to attain these special states,

[3] van Wulfften Palthe, G. Psychiatry and Neurology in the Tropics. *The Malayan Medical Journal,* September, 1933.

although heavily dependent upon local styles (*e.g.:* In the ward of one large city almost every girl had been in trance at some-time during her childhood or young girlhood. In another village trials over two months failed to find a single suitable girl who would go into trance when presented with the customary stimuli), does not seem to differ radically from such distribution in other regions where trance has been institutionalized.

There is, however, one form of Balinese trance, which appears to bear a close relationship to a definite type of childhood experience in the child-mother relationship which is characteristic of Balinese culture—this is the so-called "kris dance" which is typically brought on by a whole or partial reenactment of the *Tjalonarang* story. Careful records, in still photographs, in cinema, and verbal records, have been collected to show this basic mother-child situation in which the mother over-stimulates the young child, by setting up jealousy and coquetry situations, borrowing younger babies and suckling them to arouse jealousy, placing younger babies on their own children's heads, stimulating their children sexually, and at the same time offering no emotional rapport in return. The mother teases and flirts with the child until she produces either a state of hysterical delight or of violent weeping, and then, refusing to become involved herself, she turns casually to something else. By the time the Balinese child is three to four years old, it learns not to respond to this one-sided situation; it withdraws more and more into itself; and the basis is formed for the insulated type of personality which is typically Balinese and which fails to enter into close emotional relationship with anyone, relying instead upon ritual and art as a means of emotional expression.

In this particular form of trance, which re-enacts this childhood experience, the preface to the trance is a theatrical performance based on the story of the witch who sends pestilence upon the land; is unsuccessfully attacked by an emissary of the king of the country, who then is transformed into the dragon (*barong*)—a double mask representing a guardian supernatural

potent against disease—who, accompanied by a series of human subjects armed with krises, attacks the witch in her supernatural masked form. The witch does not fight against her attackers. She merely looks at them and they fall to the ground to rise and attack again when she takes her eye off them. They attack her in hand to hand combat, and, indifferent, relaxed, casual—even as the mother who has teased her child into a temper tantrum—she reels back and forth, unhurt by their blows, and they fall down into deep trance, to be revived into a somnambulistic state by the dragon. Then, in a frenzy, they turn their krises, which were powerless against the witch, against their own breasts, pressing the kris points against areas of skin felt to be itching intolerably. Finally they fall to the ground and go again into either a limp or rigid deep trance state from which they must be ceremonially roused with holy water and incense.

The "plot" correspondence here between the childhood experience and the special form of trance is very close and is supported by a variety of materials. But in assessing the theoretical importance of the concept of such plot correspondences it is necessary to bear in mind the probability that (1) standard traumatic experiences may occur in a culture without any such accurate ritual or artistic expression; and (2) ritual forms, originally developed or secondarily altered to correspond with the special culturally standardized childhood experience, may be borrowed and used by peoples whose cultures do not provide for any such childhood experiences. In order to establish, not the origin of the ritual in the childhood experience, but the mere fact of contemporary correspondence between childhood experience and ritual form, it is necessary to have full material on both aspects—the typical experiences of childhood and the ritual in question. There is at present no evidence to show that one can be inferred from the other. Our material suggests strongly that such correspondences as do exist can be most effectively studied by methods which record posture and gesture—*i.e.*, photographic and ciné records and also native art products.

II. *Methods of Research in Bali and New Guinea*

During the period 1936–1939 a group of investigators has been working in close cooperation in Bali:—Gregory Bateson, of Cambridge University; Margaret Mead, of the American Museum of Natural History, from March 1936 to March 1938, and during a check-up period of six weeks in 1939; Miss Jane Belo, during a number of periods of residence in Bali between 1931 and 1939, with intensive work from February 1937 to March 1938, and February 1939 to June 1939; and Mrs. Katharane Mershon, resident in Bali since 1931 and working intensively during 1938–1939.[4]

The work was financed by the following institutions: American Museum of Natural History (South Pacific Exploration Fund and the F. G. Voss Fund for Anthropological and Archaeological Research); National Committee for Mental Hygiene, through a grant from the Committee for Research in Dementia Praecox, founded by the Thirty-Third Degree Scottish Rite, Northern Masonic Jurisdiction, U.S.A.; St. John's College, Cambridge University; The Social Science Research Council (Committee on Grants in Aid); the William Wyse Foundation of Trinity College, and by considerable personal contributions from the participants in the research.

Because Balinese culture is too complex to be studied as a whole as a primitive culture might be studied, several methods of sampling were employed, by locality, by caste, and by concentrating on special rituals, special religious manifestations and special types of artistic productions.

Five localities were studied intensively, one or more of the investigators residing there: Bajoeng Gede, (simple mountain village), Bangli, (capital city of district), Batoean, (a village of high caste artists), Sajan, (a simple plains village) and Sanoer, (a district by the sea). We were assisted by trained Balinese secretaries, who were taught to record all events witnessed in Balinese and were given special training in recording verbatim conversations, in interviewing, in recording texts from dictation, etc. For the current events which were witnessed and recorded, there are at least two accounts, synchronized

[4] This central group of investigators also had the benefit of the cooperation of Dr. R. Goris, specialist in Balinese language and archaeology, Mr. C. J. Grader, specialist in the social organization of Balinese mountain communities, Mr. Colin McPhee, specialist in Balinese music and Mr. Walter Spies, specialist in Balinese arts.

on a time scale, one by a European observer and one by a native observer. For the great proportion of events which took place in the day time, there is a synchronized photographic record, combined of Leica stills and cinematograph records. For large feasts and especially important events we were sometimes able to concentrate the whole group of observers. Uniform methods of recording were used by the whole group. All details of behavior, whether verbally or photographically recorded, are placed in a known setting, against a time scale. In addition to these, cooperative records of current events, feasts, trances, theatrical performances, rites de passage, etc., special bodies of material were collected. The most significant of these are a collection of some 1200 paintings, in a recently developed style, by painters in two villages, made over a period of 2 years, comprising representations of almost every aspect of the culture, and combined with intensive study of many of the artists; a collection of some 800 contemporary carvings from a cluster of mountain villages, collected with the special intention of throwing light about attitudes and conceptions of the human body; collections of children's drawings from the localities studied intensively; and a collection of highly stylized shadow play puppets to serve as control material in comparison with the more modern art forms. In connection with these collections, there are verbal, photographic and ciné records of painters, carvers and puppet makers at work. Texts were collected of the stories upon which the paintings are based. A group of 25 young children in Bajoeng Gede was followed intensively for a period of 22 months and a check-up with full photographic records was made at the end of a further interval of 12 months more. Trancers in a number of villages were studied intensively by Miss Belo, and their records obtained on a sorting test based on Holmgren Wools, the Weigle sorting test and a Balinese object sorting patterned on the Sorting Test B as used by Dr. Bolles.[5] These tests and also the Abel Free Design Test[6] were given to a selected group of native painters. Special comparative studies were made of death rituals in a number of localities and similar cross-locality and cross-caste material was collected on other *rites de passage*. The linguistic materials include over 500 accounts in Balinese by native secretaries of events of which check records are available, and also autobiograph-

[5] Bolles, M. The Basis of Pertinence. *Archives of Psychology*, No. 212.

[6] Abel, T. M. Free Designs of Limited Scope as a Personality Index. *Character and Personality*. September, 1938.

ical texts, records of conversations, records of extemporized repartee in the theatre, trance utterances, etc.

In order to obtain comparable material from another culture, the two years in Bali were followed by an eight and a half month expedition by Mr. Bateson and Dr. Mead to the Iatmul Tribe on the Sepik River in New Guinea, previously studied by Mr. Bateson,[7] where the same methods were used, without the advantages of literate native recording, but supplemented by a great deal more verbal material reflecting the greater verbalism of the Iatmul. The same set of child training situations—suckling, sleeping, bathing, familial constellations in which sibling relationships were involved—material on posture and gesture during carving, painting, in everyday activities, and records of trance behavior were obtained for comparison with the Balinese material.

For two reasons, because of the complexity of the material and because of the war, analysis and publication of many of these results is likely to be delayed for some time. But inquiries will be welcomed from any student who wishes to acquaint himself more fully with the practical or theoretical details of any of the methodologies used, some of which were first used in the study of primitive people on a large scale in the course of these researches. All the materials are deposited in the American Museum of Natural History and are available for inspection. The ciné film used was 16 mm, exposed at 16 except where there was much action, in which case 24 exposures were used. There are 36,000 feet of ciné film and about 43,000 Leica stills.[8] Weaving Leica and ciné into a continuous record is very greatly facilitated by using lenses of the same focal length on Leica and ciné. By alternating the two methods, expense is enormously reduced and the Leicas are available for enlarging. These methods in which a verbal record of behavior over a given stretch of time is supplemented by a full photographic record, are essentially suitable for cooperative work, and cannot be used by single observers without highly trained and literate native assistants, or a very considerable modification in methodology.

PUBLICATIONS TO DATE

1. Abel, T. M. Free Designs of Limited Scope as a Personality Index. *Character and Personality*, September, 1938.

[7] Bateson, G. Social Structure of the Iatmul. *Oceania*, 1932; nos. 3 and 4. Naven. Cambridge University Press, 1936.

[8] This latter number was made possible by buying bulk film and loading into cassetts which reduces the cost to less than a third.

2. Bateson, G. An Old Temple and a New Myth. *Djawa*, publication of the Java Institute, September, 1937.
3. Belo, J. Balinese Children's Drawings. *Djawa*, publication of the Java Institute, Djotjakarta, Java, September, 1937.
4. Mead, M. Public Opinion Mechanisms among Primitive Peoples. *Public Opinion Quarterly*, Princeton, N. J., July, 1937.

Strolling Players in the Mountains of Bali. *Natural History*, New York, January, 1939.

ASTRONOMICAL CONFERENCE

OCTOBER 20 AND 21, 1939

"The Internal Constitution of the Stars"[1]

Doctor Harlow Shapley, Director, Harvard Observatory, was in charge of this meeting as Conference Chairman.

The program comprised the following papers and speakers:—

"THE DISTRIBUTION OF DENSITY IN ECLIPSING BINARIES"

Professor Henry Norris Russell, Princeton University
Doctor Theodore E. Sterne, Harvard University
Doctor Zdenek Kopal, Czechoslovakia. Title: "Density Condensations as Inferred from Ellipticities."

"SOURCES OF STELLAR ENERGY"

Doctor Hans A. Bethe, Cornell University
Doctor S. Chandrasekhar, Chicago University
Doctor G. Gamow, George Washington University. Title: "The Energy Sources of Red Giants and the Possible Causes of Cepheid Pulsation."

"OPACITY PROBLEMS"

Doctor Donald H. Menzel, Harvard University
Professor Philip Morse, Massachusetts Institute of Technology. Title: "The Effects of Chemical Composition on Opacity."
Doctor Jaakko Tuominen, Finland. Title: "Surface Opacity and Central Temperatures of Stars."

The subject of this Conference centered about the fundamental thesis proposed some years ago by President A. Cressy Morrison, to ascertain, if possible, the source of stellar energy. During the past few years, the interest in this thesis has grown to significant proportions, and the best minds in the astronomical field have been engaged upon it. It is a matter of gratification to the Academy that the investigators present included among their number the names most prominent in this field. Under the chairmanship of Doctor Shapley, the papers presented were thoroughly discussed with live interest.

[1] It is expected that a number of these papers will be published shortly in the *Annals* of the Academy (EDITOR).

It was clearly demonstrated that this method of round table consideration contributes more rapidly to a solution of many problems than any procedure hitherto attempted. It was unanimously decided to follow up this meeting with a second conference to be held next fall.

NEW MEMBERS

Added to the rolls during the summer recess from May 2 to October 2, 1939.

LIFE MEMBER

McCulloch, Warren Sturgis, M.D., Neurophysiology, Yale University, New Haven, Conn.

ACTIVE MEMBERS

Adams, George F., M.A., Instructor, Geology, Coll. of the City of New York, N. Y.

Ayres, Waldemar, A.B., Market Research, International Business Machines Corp., New York, N.Y.

Belcher, Donald, Ph.D., Fellow, Chemistry, New York University, New York, N. Y.

Briwa, Kathryn E., M.A., Instructor, Chemistry, Columbia University, New York, N. Y.

Calver, Homer N., B.S., Secretary, American Museum of Health, New York, N. Y.

Cannon, A. Benson, M.D., Associate Clinical Professor, Dermatology, Columbia University, New York, N. Y.

Conner, Robert T., Ph.D., Instructor, Chemistry, Columbia University, New York, N. Y.

Cope, Otis M., M.D., Professor, Physiology and Biochemistry, N. Y. Medical College; Flower and Fifth Avenue Hospitals, New York, N. Y.

Crossley, M. L., Ph.D., Research Director, Calco Chemical Co., Inc., Plainfield, N. J.

Degering, Ed. F., Ph.D., Associate Professor, Organic Chemistry, Purdue University, West Lafayette, Indiana.

Diamond, Moses, D.D.S., Associate Professor, School of Dental and Oral Surgery, Columbia University, New York, N. Y.

Diasio, Fortunato Anthony, M.D., Dermatologist and Syphilologist, St. Elizabeth's Hospital, New York, N. Y.

Dolley, William Lee, Jr., Ph.D., Professor, Biology, University of Buffalo, N. Y.

Drogin, Isaac, Ph.D., Chief Chemist, J. M. Huber, Inc., New York, N. Y.

Dutcher, R. Adams, D.Sc., Professor, Head, Department of Agricultural and Biological Chemistry, Pennsylvania State College, State College, Pa.

Foley, George E., Medical Technician, Diagnostic Laboratory, N. Y. State Dept. of Health, New York, N. Y.

Fremont-Smith, Frank, M.D., Director, Medical Division, Josiah Macy, Jr. Foundation, New York, N. Y.

Fries, E. F. B., Ph.D., Assistant Professor, Biology, College of the City of New York, N. Y.

Gold, Harry, M.D., Assistant Professor, Pharmacology, Cornell University Medical College, New York, N. Y.

Guizot, Electra, M.A., Technician, N. Y. State Dept. of Health, New York, N. Y.

Herrick, Gerard P., LL.D., President, Herrick Vertoplane Corp., New York, N. Y.

Hinton, James William, M.D., Associate Surgeon, N. Y. Post Graduate and Bellevue Hospitals, New York, N. Y.

Jacobson, Edmund, Ph.D., M.D., Director, Laboratory for Clinical Physiology, Chicago, Ill.

Koller, Carl, M.D., Consulting Ophthalmologist, Mt. Sinai and Montefiore Hospitals, New York, N. Y.

Konheim, Beatrice G., Ph.D., Instructor, Physiology, Hunter College, New York, N. Y.

Kuhn, Harry A., M.S., Executive Officer, N. Y. Chemical Warfare Proc. Dist., New York, N. Y.

Levin, Louis, Ph.D., Research Associate, Anatomy, Columbia University, New York, N. Y.

Littman, Julius K., M.D., Instructor, Anatomy, Columbia University, New York, N. Y.

Long, Louis, Ph.D., Instructor, Pediatrics, Columbia University, New York, N. Y.

Lorand, Sandor, M.D., Graduate, Medicine, Columbia University, New York, N. Y.

Lorch, Arthur E., Ph.D., Instructor, Chemistry, Columbia University, New York, N. Y.

Lurie, Max Bernard, M.D., Assistant Professor, Experimental Pathology, Henry Phipps Institute, Philadelphia, Pa.

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